SCIENCE

VOL. 93 FRIDAY, FEBRUARY 21, 1941 No. 2408

Ions in Gases: PROFESSOR JOHN ZELENY 167

Botanical Research by Unfashionable Technics: PROFESSOR NEIL E. STEVENS 172

Scientific Events:

The Research Section of the Rochester Academy of Science; Conference of Radio Engineers at the Ohio State University; Symposia at the St. Louis Meeting of the American Chemical Society; Symposium at Union College; Nominations of Officers of the American Institute of Electrical Engineers; Appointment of Dr. James Bryant Conant as Head of a Scientific Mission; Recent Deaths 176

Scientific Notes and News 179

Discussion:

An Important Factor in Evolution: DR. MELVILLE H. HATCH. *Linnæus on the Natural History of Man:* FRANCIS H. ALLEN. *The Soybean in China:* PROFESSOR L. CARRINGTON GOODRICH. *Our Science Meetings Again:* DR. WILLIAM SEIFRIZ. *The Presentation of Scientific Papers:* PROFESSOR J. VAN OVERBEEK 182

Scientific Books:

The Design of High Pressure Plant: PROFESSOR P. W. BRIDGMAN. *Abstract Algebra:* PROFESSOR GARRETT BIRKHOFF 185

Special Articles:

On Lubimenko Extracts of Chlorophyll-Protein: DR. M. L. ANSON. *The H-ion Concentration and the Origin of the Heart Beat:* DR. C. R. SPEALMAN. *Susceptibility to Disease in Relation to Plant Nutrition:* DR. WALTER THOMAS and DR. WARREN B. MACK 186

Scientific Apparatus and Laboratory Methods:

Histological Sectioning of Hard Tissues by a New Technique: ALEXANDER RANDALL, IV and PROFESSOR ALAN W. C. MENZIES. *The Staining of Acid-fast Tubercle Bacteria:* DR. OSCAR W. RICHARDS 189

Science News 6

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IONS IN GASES[1]

By Professor JOHN ZELENY

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The problem of size and that of mass, which is usually connected with it, has been one of great perplexity. This problem presented itself at the very outset of the study of these ions, and has remained with us ever since.

Forty-four years have passed since Thomson and Rutherford[2] adopted the ionization theory to explain the conductivity imparted to gases by x-rays. At the time, the electron had not been isolated, and the process of ionization of a diatomic molecule was regarded as consisting in the pulling apart of its two atoms.

However, when Thomson and Rutherford obtained a rough estimate of the speeds with which the ions migrate in an electric field, they found that the mobility was much smaller than an ion of atomic size should have according to the kinetic theory of gases.

[2] J. J. Thomson and E. Rutherford, *Phil. Mag.*, 42: 392, 1896.

WHAT I propose to do in this paper is to outline briefly some of the difficulties which have been met in our attempts to get a better understanding of gaseous ions, and to indicate the present state of our knowledge about these ions. And then lastly, I shall go somewhat afield to say something about the theories that have been proposed to account for lightning.

To begin, then, what is there that we should like to know about ions in gases? We should like to know their mass, size, composition and structure. We should like to know the amount of charge that each carries. And we should like to know how they differ and what part each plays in the various electrical discharges. For obvious reasons, I shall not attempt to discuss all these various aspects of the subject.

[1] Address of the retiring president of the American Physical Society given in Philadelphia, December 27, 1940.

To account for this discrepancy, these investigators proposed the explanation that, because of its charge, each ion attracts to itself a considerable number of molecules which retard its migratory motion. This idea of ion clusters did not appeal to a number of physicists.

It was soon discovered,[3] too, that in gases like air and hydrogen the mobility of the negative ions is considerably greater than that of the positive ions. A disparity between the two kinds exists in all gases, although not always in the same sense.

A new factor was thus introduced into the problem that also called for explanation.

To be sure, the usual process of the ionization of a molecule is now universally considered to consist in the ejection of one or more electrons from the molecule. Thus initially, the positive ion is a charged molecule, while the negative ion is the detached electron itself. In a few gases like nitrogen and the inert gases, the electron remains free and is highly mobile. In most gases, however, electrons of low speed readily attach themselves to molecules, and hence in these gases the negative ions, at least in their early life, also are of molecular size. The question now is, after this stage of equality has been reached, whether either or both of the ions resist further change, and, in case they do change, what is the nature of this change? The answer depends on the kind and purity of the gas used, and on other factors as well, some of which will be considered presently.

It soon became apparent that the kinetic theory of gases, as developed for uncharged molecules, is not applicable to ions without modification.[4] Owing to its charge, an ion polarizes a near-by neutral molecule, making of it a dipole. Because the field about an ion is divergent, a resultant attraction comes into play between it and such a polarized molecule, which at larger distances exceeds the other forces operative between them. Moreover, the molecules of some gases, like water vapor, for example, are normally polar, and the attraction between these molecules and ions is still greater than it is between ions and molecules, the polarity in which is produced by induction alone.

The diminution in the free paths of the ions arising from this attraction is so large that when correction for its action was taken into account in the formula for mobility, the theoretical values found for the mobility of the ions in a number of simple gases,[5] on the assumption that the ions are monomolecular, were not far from the values obtained by experiment.

Notwithstanding the disagreement in other cases, it almost seemed as if the theory of ion clusters had been dealt a knock-out blow.

However, this proved not to be the case. The number of experimental findings, which could not be explained on the hypothesis of an unchanging ion, only increased as more data were accumulated. And the argument was advanced that since the attractive forces between ions and molecules, owing to the polarization of the latter, are strong enough to deflect ions out of their paths so much as theory showed, they must also be large enough to cause permanent attachment between an ion and a molecule on close approach. It was argued that the two effects go together, that you can not have large decreases in the free paths of ions without at the same time having a high probability of attachment between ions and molecules.

Today, no one, as far as I know, contends that ions always continue in their original state. Some will only go so far as to concede a grouping of two or three molecules. Others present evidence of ions composed of as many as seven molecules. I think no one now believes that in the common gases there are any ion clusters in which the layer of molecules about the ion is more than one molecule deep.

There are some who believe that neutral molecules never adhere to ions in the sense that the molecules retain their identity but rather that such molecules as do become attached to an ion always merge with it to form a single more complex chemical molecule. I dare say at times it may be difficult to tell, without further evidence, just what the state of an aggregate is. However, it is a well-established fact that in some gases a part of the molecules are segregated into clusters even at room temperatures. Hence, it is not unreasonable to suppose that the additional attraction, arising from the charge on an ion, may enable clusters to form in some other gases.

In low pressure glow discharges accurate measurements of the masses of the ions there present can be made directly by means of mass spectrographs. These energetic discharges are a favorable seat for chemical reactions, and the measurements in a gas like air[6] actually show the presence, in greatly different amounts, of a number of different kinds of positive ions, some of which are interpreted as being compounds of the elements present.

Negative ions are found but rarely in these discharges, because the fields in them are too strong to permit the attachment of electrons to molecules. This raises the question whether clusters, which would form around positive ions in low fields, may be unable here to withstand the more energetic collisions with the molecules of the gas. However, the age of the ions in these discharges at the time of measurement, is at

[3] J. Zeleny, *Phil. Mag.*, 46: 120, 1898.

[4] P. Langevin, *Ann. de Chim. et Phys.*, (7) 28: 316, 1903; (8) 5: 245, 1905.

[5] E. M. Wellisch, *Phil. Trans.*, A 209: 249, 1909. W. Sutherland, *Phil. Mag.*, 18: 341, 1909.

[6] O. Luhr, *Phys. Rev.*, 38: 1730, 1931; 44: 459, 1933.

most but a few millionths of a second, whereas in weak fields at higher pressures large changes in mobility are at times observed to take place very much later.

There is every reason to suppose that in still more energetic discharges, like sparks, for example, the ions are all simple. The electrons are not able to attach themselves to molecules, and clusters can not form around the positive ions.

At the time of the earlier experiments on ionic mobilities in gases at normal pressures, the state of our knowledge on these matters was such that no significance was placed on the presence of small amounts of foreign molecules in the gas used. To-day, the presence of extremely minute quantities of impurities, even beyond the possibility of detection by chemical or spectroscopic means, has assumed paramount importance.

There are two principal ways in which an impurity may affect the mobilities of ions. In the first place, a foreign molecule may have a lower ionizing potential than that of the surrounding molecules. Under these conditions, the energy relations permit the transfer of the charge from the ion to the foreign molecule at close approach, provided some way is open for the removal of the excess energy which is liberated. Such collisions, though comparatively rare when the amount of impurity is small, would nevertheless in a short time transform the ions of the main gas into ions of the impurity. So what we would be measuring is the mobility of the ions of this impurity when we thought we were measuring that of the ions in the main gas.

An impurity in a gas may influence mobility measurements in a second way, provided its molecules are more polar or more easily polarizable than the molecules of the main body of gas. The possession of a high polarity might cause these molecules to adhere to ions when the surrounding molecules could not do so, and might even gradually displace less polar molecules which may have become attached to the ions previously.

The formation of ion aggregates, whether the adhering molecules are those of the main gas or those of an abundant impurity, may easily at times be completed in less than a thousandth of a second. This interval is about the shortest that can be used in mobility measurements.

Naturally, the smaller the amount of impurity present the longer will it take for the process to be completed, and this time might even run into seconds. As a matter of fact the largest variations between different observations by the same experimenter, and among those of different experimenters, are often found when extreme precautions have been taken to secure great purity in the gas used. A very slight but variable residue of some impurity might well be the cause of such disagreements.

It is easy to imagine the confusion that would result if, for example, a gas contained a very small amount of one polar impurity and a still smaller amount of a second more polar impurity. The monomolecular ions might first quickly form aggregates composed of molecules of the main gas, which would gradually be transformed into clusters composed chiefly of the less polar and more abundant impurity, only to be changed finally into clusters of the more scarce and more polar contamination.

The mobility observed may thus depend not only upon the kind and amount of the impurity present but also upon the time after the formation of the ions that the measurements are made.

When the method used had a high resolving power the mobility spectrum has shown two distinct mobility peaks[7] in a given gas under some conditions and only one under others. Can this be explained otherwise than on the assumption of a change in ion aggregates?

Older ions are often found to be present in an unresolved group,[8] the mobility of the fastest of which may be 25 per cent. or more higher than the mobility of the slowest ones. For this reason disagreement between different observers has at times arisen, since some of the methods used measure the mobility of the slowest ions present, others that of the fastest ions, and still others the mobility of the most numerous type.

The presence of a small amount of water vapor in air reduces the mobility of its negative ions[9] and at the same time increases that of the positive ions. Molecular aggregates about the ions are again indicated.

When ions produced in one gas are passed through a different gas their mobility is usually found to depend only on the gas through which they are passing[10] and is independent of the gas in which they originated. Since an exchange of charge from the ion to a molecule of the second gas is ruled out in a special experiment with radioactive ions, we must again seek an explanation in some kind of cluster formation.

Enough has been said to show the unsatisfactory condition that existed a few years ago, where so much depended on conjecture and where suspicion was thrown on the credentials of every ion that had thus far been investigated. Something drastic needed to be done if experiment was to be a true guide to theory

[7] H. A. Erikson, *Phys. Rev.*, 17: 421, 1921; 26: 465, 1925.

[8] M. LaPorte, *Ann. de Phys.*, 8: 466, 710, 1927; J. Zeleny, *Phys. Rev.*, 34: 310, 1929; J. L. Hampshere, *Proc. Roy. Soc.*, 127: 298, 1930.

[9] J. Zeleny, *Phil. Trans.*, A195: 193, 1900; *Phys. Rev.*, 36: 35, 1930.

[10] E. Rutherford, *Phil. Mag.*, 5: 95, 1903; A. Blanc, *Jour. de Phys.*, (4) 7: 825, 1908; E. M. Wellisch, *Proc. Roy. Soc.*, 82: 500, 1909.

and in turn theory be a guide to further experiment. It seemed manadatory, if ions of assured identity were ever to be made available, that the most unusual care must be taken both in the purification of the gas used and in the outgassing of all parts of the measuring apparatus.[11]

Experiments under such conditions have been carried out in recent years with interesting results. To begin with, the mobility of the positive ions in helium was now found to be about four times as large as that reported previously.[12] Especially significant are the measurements made of the mobilities of the positive ions of the five alkali metals when moving through the five inert gases.[13] The reason for the choice of these substances was that the ionizing potentials of the alkali metals are in all cases lower than those of the gases used, so that the metal ions are not able to transfer their charges to the molecules of the gas through which they are passing. Contrary to previous results, the mobilities of the different ions now differed widely among themselves when passing through the same gas. The mobilities diminished in a regular manner with increase in the atomic weight of the ions used, decreasing for example in the gas krypton from 4.03 cm/sec in a unit field for the lithium ion of atomic weight 7, down to 1.44 for the caesium ion of atomic weight 133.

The above values are in fairly satisfactory agreement with those computed by Langevin's formula[14] for mobility, which was derived on the simple assumption that molecules are spherical, elastic and polarizable. The largest disagreement is for helium where the observed values are about 20 per cent. higher than the theoretical ones.

These results are encouraging because, from the regularity of the relations, there is a very high degree of probability that the ions here in question are in reality all monatomic.

A number of other revealing experiments were done with these alkali ions in the inert gases. For example, when lithium ions were passed through these gases at room temperatures, and *low fields were used* in making the mobility measurements, another set of ions appeared in addition to the ones noted above.[15] The mobility of these in xenon, for instance, was but little more than a quarter of that of the faster ions. When the temperature was now reduced to a very low value, the faster group disappeared altogether and the slow ions alone were detected in all the gases, except helium. Hence, these slow ions must result from the attachment of molecules, of the inert gases themselves, to the lithium ions. Now this is very interesting. The noble gases are called inert gases because they are chemically inert. Yet here they are adhering to the alkali ions. The union is not likely to be a chemical one.

Taking one more example; a small amount of water vapor added to any one of the inert gases, reduced the mobility of all the alkali ions through it.[16] The lighter ions lost most, so that now the mobilities of all of the ions were roughly equal. It is natural to suppose that the decrease of mobility was due to the attachment of water molecules to the ions, and an estimate of the upper limit of the number of molecules so attached to any ion can be made on the supposition that the change of mass alone affects the mobility, although actually the size of the ion is also a factor. A clustered lithium ion in argon had almost the same mobility as a caesium unclustered ion, and, on the supposition made, it must therefore have a mass of 133 units, the same as that of caesium. If the increase of 126 units in the mass of the lithium is due to water molecules then a simple division shows that there must be seven of them clustered about the ion.

When the strength of the field used in these measurements which were done in the presence of water vapor was gradually increased, the mobility rose to that of the unclustered ion. The aggregates could no longer withstand the more energetic collisions with the molecules of the gas, and so the ions remained monomolecular.

However, the information furnished by the use of this improved technique, valuable as it is, is mainly qualitative. We must remember that it is not possible to determine the exact masses of ions from their mobilities alone, because the size of the ion is always involved as well. Unfortunately, too, theory shows that mobility values respond less and less to changes in the ion's mass as this mass gets larger and larger.

Furthermore, energy relations permit a molecular ion, on close approach to an uncharged molecule of the same kind, to give up its charge to the uncharged molecule. This exchange would reduce the mobility of the ion. However, the probability of such an exchange in any gas is still unknown. Much remains unknown, too, about some of the energy states in molecules and about the forces arising from them. Until more exact knowledge on some of these points is obtained we must not expect further refinements in the mobility formula as it applies even to monomolecular ions.

Still less can theory give us an answer as to the kind of ion cluster, if any, that can form in any gas under given circumstances, nor as to the connected problem of accounting for the differences between the

[11] L. B. Loeb, *Phys. Rev.*, 36: 152, 1930.

[12] A. M. Tyndall and C. F. Powell, *Proc. Roy. Soc.*, 134: 125, 1931.

[13] A. M. Tyndall and C. F. Powell, *Proc. Roy. Soc.*, 136: 145, 1932; C. F. Powell and L. Brata, *Proc. Roy. Soc.*, 138: 117, 1932.

[14] Reference 4.

[15] R. J. Munson and K. Hoselitz, *Proc. Roy. Soc.*, 172: 43, 1939.

[16] R. J. Munson and A. M. Tyndall, *Proc. Roy. Soc.*, 172: 28, 1939.

positive and the negative ions. A goodly array of precise experimental data obtained under simple but definitely known conditions may, on the other hand, help lead the theorists to a more exact formulation of the laws of intermolecular action.

Our atmosphere is slightly conducting owing to the incessant ionizing action of cosmic rays and the radiations from radioactive substances, to which at high altitudes must be added the radiation from our sun.

Many of the ions thus produced become attached to certain large nuclei which are always present in the air, and predominantly so in the neighborhood of cities. The small ions are thus changed into so called large ions and some of these have diameters a hundred times that of a nitrogen molecule.

Since these ions do not recombine readily, owing to their slow thermal motion, there may be as many as 50,000 or more of them in a cubic centimeter in some places, which is greatly in excess of the number of small ions present.

The nuclei to which the small ions become attached appear to be invisible droplets of water, which have formed around some hygroscopic substance that is in all probability a product of combustion.

The positive ions always predominate in the atmosphere, and for this reason there is a vertical electric field near the earth's surface which in clear weather amounts to about 100 volts per meter. The atmospheric ions moving in this field constitute a downward current of over 1,000 amperes taken over the whole of the earth's surface. This current must somehow be neutralized. The most common belief is that this is accomplished mainly by lightning and by point discharges from objects on the earth.

The question arises by what process the two electricities are separated to produce the large differences of potential necessary for both of these kinds of discharge, potentials which may reach a billion volts.

It is generally agreed that the work of separation is done mainly by the wind and by gravity acting on charged rain drops as they fall. There is much dispute however as to how the drops get charged in the first place.

Several processes have been proposed to account for the charges on these drops, none of which has received general support.

I want lastly to consider the central ideas of some of these theories, leaving aside all details of application.

Gunn[17] has recently suggested that the small ions in the air are the source of these charges, these ions behaving like water molecules because they have water molecules attached to them. As I understand the argument, in the upper portion of a cloud to which ascending winds have brought moisture laden air, water vapor is condensing onto the rain drops and the flow of the vapor is naturally toward the drops, and so there the ions must also diffuse in the same direction. As the negative ions diffuse the faster, the drops will here become charged negatively. On the other hand, drops falling in the unsaturated air near the bottom of a cloud are evaporating and so the flow of vapor is here away from the drops, and as the diffusion of the ions again partakes of this flow, so the argument states, the drops will now become charged positively. In both cases each drop is surrounded by a volume of gas having a charge opposite in sign to its own. As the drops fall and air currents carry away the charged gas, large differences of potential may in this way be developed between the two portions of the cloud, and between the cloud and earth as well.

What I can not follow in the argument and this may not be decisive against the theory, is the idea that the diffusion of ions must follow the concentration gradient of the water vapor. The ions will establish their own concentration gradient, which in both cases will be toward the rain drops where alone they are being absorbed. There is never any force driving a molecule with or against a concentration gradient. Every molecule and so also every ion, in its wanderings, is a law unto itself, and moves about independently of what neighboring molecules may be doing. Changes in concentration that may take place with time are determined solely by statistical considerations.

I feel that the experimental data by which Gunn supports his theory, and which he states were obtained under not well-controlled conditions, need verification as regards the positive charge he found was acquired by a water drop, when placed in an unsaturated atmosphere of ionized air.

C. T. R. Wilson[18] has postulated a different process by which rain drops get charged, this time from the large ions in the atmosphere. Rain drops are normally charged by induction in the earth's field, positively on the lower side and negatively on the upper side. The terminal velocity of fall of the drops is greater than the downward speed of the large positive ions moving in the earth's field. These positive ions therefore can not overtake the drops from the rear and such of them as a drop overtakes are pushed aside by the positive charge on the bottom of the drop.

The upward moving negative ions, however, are attracted by the positive charge on the lower side of the drop which thus acquires a negative charge. The fall of these negatively charged drops from the upper to the lower part of a cloud gives this part a negative charge and leaves the upper portion charged positively by the ions which were left behind. After the

[17] R. Gunn, *Terr. Mag.*, 40: 79, 1935.

[18] C. T. R. Wilson, *Jour. Frank. Inst.*, 208: 1, 1929.

drops have fallen far enough, the field between the earth and the bottom of the cloud is thus reversed, in confirmation of what experiment shows. The polar character of the cloud's charges is also in agreement with experiment. An objection that I have to this theory, to which its advocates have not given a satisfactory answer, is that it completely ignores the great air disturbances which accompany all thunderstorms, and which, it would seem, must in some way be one of the essential conditions for lightning production. According to the process postulated, lightning should be as likely to occur during a gentle shower as during a turbulent storm; in cold weather as in hot; near the Pacific coast as often as near the Atlantic. But such is not the case.

Another theory that has attracted much attention is that of Simpson.[19] This theory does not make use primarily of either the small or the large ions already present in the atmosphere. It postulates that the storm manufactures its own ions. It is well known that when a water drop is disrupted by an air blast, for example, the droplets which result from the disruption are found to be positively charged while an equal negative charge appears on ions in the air. Simpson seized upon this process as the one active in a thunderstorm. Swiftly ascending currents of air in the front part of the storm meeting falling rain drops tear them asunder with the resulting separation of electricities that has been described. The positively charged droplets may in time grow by coalescence, only to be disrupted again on reaching a certain size. Eventually, the drops fall away from the turbulent region of the cloud, the negative ions are carried upward by the air currents, and the high potential differences requisite for lightning are gradually built up.

This theory has a strong appeal because it makes use of the high vertical winds characteristic of thunderstorms to generate the large charges found in them. The comparative lack of lightning during cold weather and its entire absence in the Arctic regions may have a possible explanation on this theory. The argument is like this. After the wind has pulled a part of a drop into a film, the film breaks and snaps back into a number of droplets. These are charged positively because, in jerking back, the film loses some of the negative charge from a double layer supposed to exist on its surface. The faster the film contracts the greater its loss of charge. The speed of contraction depends largely on the viscosity of the liquid, and this decreases rapidly with rise of temperature, so that a warm film will contract faster than a cold one. Experiment has confirmed this reasoning,[20] for it was found that the charges obtained by disruption of drops falling through an air blast increase rapidly as the temperature of the drops is raised.

Hence we should expect potentials sufficient for the production of lightning to be reached with less violent air currents when the drops are warm than when they are cold. The only question that remains open is whether at the altitudes where the drops are being disrupted, the temperature is really different when it is undergoing large changes at the earth's surface.

The main argument directed against Simpson's theory is that the charges predicted by it in the clouds are more often than not of the wrong sign. Simpson[21] has countered these objections, but the arguments on both sides are too involved to be considered here.

The conditions in a storm are complicated and seem to vary with time and place, and the information about the actual distribution of the charges in a thunderstorm is still meager indeed. A more accurate knowledge of the location of these charges would be helpful in reaching a decision between the various theories, although in the end it may turn out that more than one of the processes which have been described, play some part in the phenomenon of lightning production.

[19] G. C. Simpson, *Phil. Trans.*, A 209: 379, 1909.

[20] J. Zeleny, *Phys. Rev.*, 44: 837, 1933.

[21] G. C. Simpson, *Proc. Roy. Soc.*, 114: 376, 1927.

BOTANICAL RESEARCH BY UNFASHIONABLE TECHNICS[1]

By Professor NEIL E. STEVENS

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NEARLY two years ago in suggesting that among certain crops in the United States there was a relation between disease damage and pollination behavior, I ventured to use volume of publication on the diseases of these crops in relation to their farm value as a measure of the commercial importance of diseases in their culture.[2]

The main thesis that, at least among the crops classed as "grains," disease losses are much more important in those groups which are wholly or largely self-pollinated than in those which are largely cross-

[1] Address of the vice-president and chairman of the Section for the Botanical Sciences of the American Association for the Advancement of Science, Philadelphia, December 29, 1940.

[2] SCIENCE, 89: 339–340, 1939.

pollinated, has not yet been attacked. Indeed, if we are to take the teachings of the breeders seriously and actually credit the existence of selection as a vital force rather than just something in a book, it could hardly be otherwise.

On the other hand, a good many have found a source of amusement in the type of evidence offered. A characteristic comment has been, "this is interesting but not very scientific." Now, when some one makes the comment of "interesting but not very scientific," I find myself somewhat in the position of John Wesley regarding music. When some of the orthodox complained that the songs which thousands of his followers were singing throughout England were not sacred music, he replied with the question, "Why leave all the good tunes to the devil?" Why should we in our search for evidence on live problems leave all the interesting fields to the economists?

The general thesis advanced is, in view of the present great interest in breeding for disease resistance, of real, even basic, importance. Such generalizations are dangerous enough and hard to prove, but the available evidence should at least be considered on its merits and without any attempt to find too difficult an explanation. It is in dealing with just such general observations that present-day biologists lay themselves open to the criticism once leveled at a well-known writer by the late Newton D. Baker that he "discards the obvious as unreasonable and embraces the unreasonable because it is not obvious."[3]

In this case the inference drawn seemed to be in line with the general impression among informed agronomists, yet the suggestion was largely disregarded because of the unusual source of the figures presented. This question then has very much broader significance, which is my justification for bringing it up here, a significance as wide as the whole field of biological investigation. For it is only one phase of the more general question, Shall we permit easy and fashionable methods to determine our lines of research, or shall we attack interesting and important problems with the best tools we can find or devise?

Since the publication of the earlier article I have examined three other possible sources for evidence on this point and find them in essential agreement. For example, in a series of articles discussing progress and possibilities in plant and animal breeding, prepared by specialists of experience and high standing in their respective fields, and published in the United States Department of Agriculture Yearbooks for 1936 and 1937, relatively much more space was given to discussing disease resistance in wheat, oats and barley, all largely self-pollinated, than in corn, which is, of course, cross-pollinated. The relative amounts expressed as percentage of the total space which was given to this phase were: wheat, 11.5; oats, 10.5; barley, 7.0; corn, 1.9. Moreover, the disease loss estimates compiled and published by the United States Department of Agriculture from figures sent in by thousands of crop reporters in all parts of the United States are in agreement with the foregoing. The estimated average annual reductions for the decade 1916–1925,[4] which are slightly higher than for the earlier years, but fall in the same order, are in percentage: wheat, 5.2; oats, 2.8; barley, 2.7; corn, 0.4. Rye is not mentioned. Finally, the estimates compiled by the Plant Disease Survey for the years 1917–1937, inclusive, show a striking agreement with the foregoing. This, of course, raises the question of the significance of these estimates, or indeed any estimates relating to plant life.

Three years in the Plant Disease Survey followed by four seasons of renewed contact with cranberry growers and their problems led to much consideration of estimates and their place in biology, with conclusions which may be worth sharing, in spite of, or perhaps because of, the fact that they may not agree with current fashionable concepts.

First of all, what is an estimate? An estimate, according to Webster, is a "judgment of opinion, usually implying careful consideration or research—a judgment made by calculation, especially from incomplete data." This definition applies well to both estimates of crops and estimates of crop losses. Comparison of the actual shipments of cranberries by the New England Cranberry Sales Company with the estimates turned in by the members in September, only a short time before the harvest begins, over a period of 24 years showed errors of 5 per cent. or over in one or both of the two major varieties in eighteen of the years and errors of 20 per cent. or over in six years. Moreover, in most cases the causes of the errors seem to have been psychological rather than observational. The inherent caution of real New Englanders appears in a decided tendency to underestimate the Early Blacks, the variety first harvested. A conspicuous cause of error appears also in the influence of the previous crop. Very large crops tend to be greatly underestimated if they follow small crops and small crops to be greatly overestimated if they follow large crops. May I hasten to add that these serious, demonstrated errors led to no suggestion on the part of the cranberry growers that estimates be dropped. They apparently agreed with Poincaré that "it is far better to forsee even without certainty than not to foresee at all."[5]

It may not be impertinent to add that only academic minds question the usefulness of estimates. Businesses

[3] *Yale Review*, 30: p. 40, September, 1940.

[4] "Crops and Markets," Vol. 3. Supplement 10, pp. 321–322, October, 1926.

[5] "The Foundations of Science" (Authorized Translation by G. B. Halsted), Science Press. 1913.

are projected, factories built, professions chosen and crops planted on the basis of estimated future needs. Money is loaned, houses are purchased, horses are bought and are (or used to be) swapped on the basis of estimated value. Husbands are selected, married and, with increasing frequency nowadays, also swapped on the basis of estimated present or future worth. Many scientific investigators, on the other hand, only with the greatest reluctance, hazard their reputations (to say nothing of their money) on even the most carefully qualified estimates, preferring to devote their attention to phenomena which may be "measured."

Admittedly—indeed by definition—a high degree of numerical accuracy can not be expected of estimates. If in the case of the crop estimates just cited, a group of highly intelligent and vitally interested cranberry growers dealing with quantities which could be rechecked very soon after they were estimated, made frequent errors of 20 per cent. or over, there are probably very much larger errors in our crop loss estimates. The real question is, are they adequate for or at least the best available means of serving their purpose?

The purpose of crop estimates is clearly understood by all. It is to furnish those concerned in handling the crop with the most nearly accurate advance information possible. What is the purpose of the estimates of crop losses? Primarily, I take it, to furnish some basis for the comparison of one year with another, one region with another and one disease with another. So far as can be judged from reading the introductions to the various published summaries of estimates it was recognized from the first that they could not be very accurate. They were not measurements, they were "judgments of opinion." Into such judgments certain psychological factors undoubtedly enter as causes of errors. There has been, I believe, a general tendency on the part of most collaborators to err on the side of caution and to underestimate losses from diseases. This may be in part a reaction from the fantastic estimates of years ago. Certainly in some cases where it has been possible to measure the effects of disease, the figures have been much higher than the usual estimates. Study of the estimates over a period of years leads me to the conclusion that there is some irregularity as a result of special interest or knowledge, and that estimates are influenced by general interest or lack of interest in a disease. Local prejudices certainly seem to influence some of the estimates, and it is charged that there is some deliberate distortion.

It might be no less than fair also to call attention to the fact that estimates differ in degree only—not in kind—from many of our so-called measurements. That there are always subjective sources of error, and that in general, the larger the problem and the wider the field studied, the greater the probability of error.

It is my impression, gained through several years of study and observation, that the tendency to discredit the estimates of crop losses results from the inclination to expect of them wholly unattainable degrees of numerical accuracy, and that this tendency is fostered by the fact that the estimates are expressed in numbers. If no claim is made to numerical accuracy in the crop loss estimates, why are they numerically expressed? Simply because of the limitations of any other method of expression. The case is admirably summarized by Sorokin in a discussion[6] of methodology in another field. His arguments, which seem to me unanswerable, are that the numerical method is more concise and economical, and that verbal quantitativism has a very limited number of gradations. A glance at Table I which contrasts actual published figures with attempted verbal equivalents, will illustrate the point. After all, we have learned to use figures with discretion in some fields. If a text we use in 1939 states that certain plants grew a million years ago, we do not say in 1940 that these plants grew a million and one years ago.

TABLE I

ESTIMATED LOSSES DUE TO BACTERIAL WILT IN SWEET CORN IN PENNSYLVANIA

Year	Numerical (percentage)	Verbal quantitative
1931	..	did not notice it
1932	45	a massacre
1933	25	heavy losses
1934	20	less heavy losses
1935	15	still less heavy losses
1936	8	losses still less heavy but still heavy
1937	3	losses light but still noticeable

In view of all their admitted inaccuracies, are crop loss estimates worth while? I believe the answer to this question should be based on two further questions—How important are the problems to which they relate? Are they the best tools available? Obviously they are our only available means of studying the fluctuations in the intensity of many diseases from year to year. Just how important this is depends, of course, on one's judgment of the scientific and practical importance of epidemiology.

In this field, at least, I admit myself a prejudiced witness, for I am deeply interested in the history of plant diseases. On the basis of estimates collected by the Federal Government, some of them before the Department of Agriculture was organized, I was able to trace the spread of potato blight in 1843, '44, '45[7] and to publish for the first time a record of the

[6] P. A. Sorokin, "Social and Cultural Dynamics," Vol. 2, p. 22.

[7] *Jour. Washington Academy of Sciences*, 23: 435–446, 1933.

extent of losses from the disease in another great rot period of 1885–1886.[8] I confess myself not greatly concerned by the fact that New York records the loss in 1885 as 38 per cent. and adjacent Vermont as 17 per cent., nor do I care whether either figure is accurate. The fact is that we are able to learn from them that the losses in both states were large in 1885 —much larger than in the subsequent year.

At some risk to what little may be left of my scientific reputation I wish to add that for a study of the fluctuations of disease from year to year, estimates may be just as useful as figures based on laborious measurements and calculations. A figure published a few years ago[9] gives some information about the incidence of fruit rots of cranberries on Cape Cod during 5 years. It is based on the results of storage tests of eight to ten lots of the most important cranberry variety in Massachusetts taken from the same bogs and from nearly the same sections of these bogs year after year. The lots were adequately sampled, carefully sorted and accurately counted. For purposes of comparison they show that the crops of 1931 and 1933 were of very poor keeping quality, the crop of 1932 very good, and the crops of the two other years good but not exceptional. These facts were already well known to inspectors, sales agents and growers throughout the area on the basis of their own experience and observation long before my tests were concluded.

There is another and very practical angle to the study of plant disease control in which estimates of disease, yes, and forecasts of the probable incidence of disease, are of direct significance. How else can we evaluate our control measures? I have already quoted more than once the reply I received when a few years ago I asked a plant pathologist who had spent several years in the study of the control of damping-off in forest nurseries for a brief statement of his recommendation for disease control. His first sentence was "If your loss is less than 15 per cent., forget it." I think it would be an excellent practice and would tend to establish us in the eyes of practical men if all our recommendations for disease control began, "If your loss is less than — per cent., forget it." In order to determine what this figure should be, we have at present no other source of information than estimates of plant disease losses. However interesting it may be to work out theoretical controls, in the actual practice of plant pathology we have no right, outside the field of ornamentals, to recommend the use of any control measure which does not cost demonstrably less than the probable loss from the disease. On a very much larger scale, how is it possible to evaluate the results of campaigns for the eradication of long-established diseases, for instance, fire blight and peach yellows, unless or until we know something about the extent of the fluctuations in these diseases when there have been no eradication campaigns?[10]

The difference of opinion as to the value of crop loss estimates would seem to boil down to this: a difference of opinion between those whose first concern is an alleged mathematical accuracy and those who are seriously concerned with practical disease control and adjustment to disease conditions in a practical world. For my own part, I can see little justification for a plant pathology—certainly not for a plant pathology supported by public taxation—which is more concerned with methodology than with objectives.

I should like to go even further than this and insist that we are in serious danger of needlessly limiting our interests and usefulness, to say nothing of making ourselves ridiculous, by insisting on professionally ignoring all phenomena which do not lend themselves readily to measurement by fashionable technics.

Fungus spores can be measured in fractions of microns and the geographic ranges of fungi only approximately determined, while the causes of their distribution can be little more than guessed at. Shall we, therefore, spend our lives measuring spores and neglect questions relating to distribution and its causes?

Because it is impossible to determine whether the loss caused was 18.2 per cent. or 22.4 per cent. must we refrain from recording the fact that *Diplodia zeae* caused severe losses in Illinois in 1938?

Shall we fearfully protect our precious reputations by refusing to publish any suggestions or opinions not bolstered with measurements which seem to be statistically respectable?

Through the indulgence of the Wisconsin Academy of Science, we have been able to place in print a conviction held by myself and two associates that attempting to utilize alkaline flooding water in the cultivation of cranberries in that state is apt to lead to financial disaster. Of this, as yet, we have not a shred of experimental evidence, nor is there any chance of getting that within the next few years. We know, however, that of the properties now under cultivation those with alkaline water are the hardest to manage successfully. We know that an entire cultural area, once the largest in the state, which has alkaline water, is now almost abandoned; while another with acid water is now cultivated by the third generation of successful cranberry growers. We know that the only property in the earlier area still under cultivation is in the red and has been in deep red for at least a quarter of a century. We have case history after case history, some running back 45 years.

[8] *Phytopathology*, 24: 76–78, 1934.

[9] N. E. Stevens and J. I. Wood, *Bot. Rev.*, 3: 277–306, Fig. 18, 1937.

[10] *Jour. Econ. Entom.*, 31: 39–44, 1938.

In spite of all this evidence some of our colleagues (fortunately not the cranberry growers themselves) find it hard to take the conclusions seriously. If, in contrast to the above, we were reporting results obtained from six 4-inch pots for one year, our results would be far more respectable.

A realization of the possibility that even in science there is danger from too devout worship of the fetish of alleged accuracy has recently been found in papers from two fields which are commonly supposed to be much more characterized by accuracy than biology can reasonably hope to be; namely, astronomy and physics. Professor Henry Norris Russell, in his presidential address before the American Astronomical Association[11] in 1937, discussed "the place, utility, and limitations of approximate methods in astronomical work."

Professor Russell's paper is short enough to be easily read and too compact to be easily abstracted. He gives a number of instances in which approximate methods have given highly significant results, and raises the important point of what he calls "astronomical economics," that is, the question as to the extent to which the director of a great modern observatory should spend money and energy in securing more and more accurate observations. He indicated that spending effort and funds for accuracy is justified to the extent that the problem under study requires accurate measurements for its solution.

Even in astronomy it appears there are those who would let methods dictate problems. He quotes E. C. Pickering as saying that shortly after he had become director of the Harvard Observatory he was severely and publicly criticized by a conservative group because "instead of putting his time on meridian observations, which can be made with an accuracy better than one part in a hundred thousand, he is working on photometry, with errors of ten per cent. or worse, and in spectra, with no accuracy at all." And, he adds, "Pickering had the courage of his convictions, and kept on with the results that we know."

Charles Galton Darwin in a discussion of "Logic and Probability in Physics"[12] says: "What is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges; and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline."

A drastic change in my surroundings a few years ago led to my reading a number of books in the field of sociology—which is after all a sort of biology. I find these workers are frequently faced with this same choice, relatively accurate measurements in a less interesting and significant field or obviously crude measurements in a highly interesting one. May I then close with quotations from two of them, "We have been choosing the problems of study not so much by their importance as by a possibility of making a 'fine and accurate study of a topic.' . . . Pushed too far in that direction, these investigations become a worthless parody on science. To avoid this situation, once in a while, somebody has to take upon himself the doubtful privilege of selecting an important topic for his study, though it does not lend itself to an exact investigation."[13] "Method must conform to material and not vice versa . . . the first loyalty of a scientist is to his material; . . ."[14]

SCIENTIFIC EVENTS

THE RESEARCH SECTION OF THE ROCHESTER ACADEMY OF SCIENCE

A NEW Research Section of the Rochester Academy of Science has been organized under the constitution of the academy. Meetings will be held on the first Tuesday of each month giving opportunity for social and professional discussion.

The Research Section has as one of its objects the promotion of the more active publication of the *Proceedings* of the academy. Its members pay an annual fee of $3, in addition to the fee of $2 for general academy membership. The additional $3 will be used entirely for the benefit of the *Proceedings*. It is planned to issue from two to four numbers annually, four numbers of about 200 pages constituting a volume. The subscription price will be $5 a volume, with a guarantee that the cost will not exceed this sum.

The first meeting of the section was held on January 13. Karl Patterson Schmidt, director of the division of zoology of the Field Museum of Natural History, Chicago, gave a lecture entitled "Desert and Highland in Peru," which was devoted to a résumé of impressions gained while on an expedition to Peru in 1939. In his introductry remarks he presented his views on the value and purpose of an Academy of Science. The second meeting, on February 4, provided a round-table discussion on "Speciation in Plants and Animals" conducted by Professor Sherman C. Bishop and Dr. Richard Goodwin, both of the department of biology of the University of Rochester. The third meeting will be held on March 4, when William A. Ritchie, of the Rochester Museum of Arts and Sci-

[11] H. N. Russell, *Publications Am. Astron. Soc.*, 9: 108–114, 1938.

[12] SCIENCE, 88: 155–159, 1938.

[13] P. A. Sorokin, "Social and Cultural Dynamics," Vol. 2, p. 270.

[14] John Dollard, "Caste and Class in a Southern Town."

ences, will present the results of his excavations on Frontenac Island, Cayuga Lake, during the summers of 1939 and 1940.

Professor David Goddard, professor of botany at the University of Rochester, is chairman of the Research Section. Other members of the executive committee include Professor Bishop, Professor Floyd C. Fairbanks, professor of astronomy at the university and president of the academy, Dr. Dean L. Gamble and Dr. R. L. Roudabush, both of Ward's Natural Science Establishment, and W. S. Cornwell, recorder.

CONFERENCE OF RADIO ENGINEERS AT THE OHIO STATE UNIVERSITY

THE fourth annual Broadcast Engineering Conference, directed by Dr. W. L. Everitt, of the Ohio State University, which lasted for ten days opened on February 10 for the discussion of new developments in the field of color television, ultra-high frequency transmission, etc.

During the first week speakers reported on technical improvements of speech input systems, studio acoustics, polyphase broadcasting, receivers, loud speakers, sound reproduction from recording and the status of television. Dr. Peter C. Goldmark, head of research in television of the Columbia Broadcasting System, discussed his work on color television, and E. K. Jett, chief engineer of the Federal Communications Commission, spoke at a dinner session on "Communications in National Defense." A. D. Ring, assistant chief engineer in charge of broadcasting for the Federal Communications Commission, led a general question period during the first week's program, which was sponsored jointly by the department of electrical engineering of the university and the National Association of Broadcasters.

Sessions held during the second week's program, which opened on February 17, considered the status of frequency modulation, its operating problems and technical aspects. Lectures were given on ultra-high frequency broadcasting. Homer Dudley, of the Bell Telephone Company, specialist in design and construction of speech analyzing and synthesizing circuits, spoke on "The Vocoder or Remaking Speech Electrically" on the evening of February 18.

A new feature of this year's conference was the addition of laboratory experiments on new equipment and techniques in broadcasting station measurements. Each member had an opportunity to make broadcast station measurements. The laboratory periods were held in the evening in several sections, so that all attending could make tests in which they are particularly interested. The scope of the conference was broadened this year to include topics of interest to the engineers working with airway and police communications, general receiver and laboratory development.

SYMPOSIA AT THE ST. LOUIS MEETING OF THE AMERICAN CHEMICAL SOCIETY

SCIENTIFIC problems involved in national defense, including the production of synthetic rubber and other materials and the fortifying of food with minerals and vitamins, will be discussed at the one hundred and first national meeting of the American Chemical Society, which will be held in St. Louis from April 7 to 11. An attendance of 3,500 is expected.

Dr. Charles L. Parsons, Washington, D. C., is the national secretary of the meeting. Dr. LeRoy McMaster, Eliot professor of chemistry and head of the department at Washington University, has been named honorary chairman. H. Edmund Wiedemann, consulting industrial chemist and past national president of the Food and Drug Officials Association, has been appointed general chairman.

Fourteen symposia, representing virtually every field of chemistry, in addition to technical papers to be presented before seventeen of the eighteen professional divisions, are planned. Several addresses by well-known scientific men will be delivered at a general session on Monday, April 7, in the St. Louis Municipal Auditorium. Dr. William Lloyd Evans, of the Ohio State University, president of the society, will preside.

"Fortifications of Foods with Minerals and Vitamins" is the subject of a group of papers to be presented in the Division of Agricultural and Food Chemistry, of which Gerald A. Fitzgerald, of the General Foods Corporation, New York City, is chairman. Another session of the division will be devoted to "Fats." The Biological Division, of which Dr. Herbert O. Calvery, of the U. S. Food and Drug Administration, is chairman, will hold its semi-annual symposium on "Vitamins."

"Some Current Projects in Medicinal Chemistry," "New Engineering Technique," "New Developments in the Processing of Rubber" and "Phenol-Formaldehyde Resins and Plastics" are other divisional symposia which will bring the emphasis of science to bear upon questions related to national defense.

The Division of Petroleum Chemistry, which is celebrating the twentieth anniversary of its founding, will discuss "Analytical Methods Used in the Petroleum Industry."

"Smokeless Fuels and Air-Pollution Abatement" will be the subject of a session of the Division of Gas and Fuel Chemistry. Other symposia will deal with the nomenclature of inorganic chemistry; thermodynamics of electrolytic dissociation; contemporary work in the monosaccharide field of sugar chemistry; the last two years of college chemistry, and a student symposium in chemical education.

Divisional papers will also be presented in the fields

of analytical and microchemistry, cellulose, colloid, industrial and engineering, medicinal, organic, paint, varnish and plastics, physical and inorganic, and water, sewage and sanitation chemistry and in the history of chemistry.

The chemists will hold a banquet on April 9 at the Hotel Jefferson, which has been designated as headquarters for the meeting. Divisional sessions will take place in the Municipal Auditorium, where registration will open on Sunday afternoon, April 6. The council, composed of representatives of the ninety-three local sections throughout the country as well as of the national officers, directors, editors of publications, past presidents and divisional chairmen, will convene on Monday morning. An elaborate social program is planned and numerous trips of inspection will be made to the industries of the St. Louis area.

SYMPOSIUM AT UNION COLLEGE

A SYMPOSIUM devoted to the general subject "Science Views Man" in which some of the leading scientific men of the country will take part and to which members of colleges, universities and industrial research laboratories in the New England and Middle Eastern Atlantic States have been invited, will take place at Union College on March 21 and 22. Dr. Edward Ellery, professor emeritus of chemistry and national president of the Society of the Sigma Xi, states that the object of the symposium is "to present man as science views him, in contra-distinction to the moral and religious point of view."

Dr. A. J. Carlson, Hixon professor of physiology at the University of Chicago; Dr. Bronislaw Malinowski, professor of anthropology at Yale University; Dr. Willis R. Whitney, organizer of the Research Laboratory of the General Electric Company and now vice-president in charge of research; Dr. Kirtley F. Mather, professor of geology at Harvard University, and Dr. Harlow Shapley, Paine professor of astronomy and director of the Harvard Astronomical Observatory, will be among the speakers.

According to the official announcement "they will trace the impact of several sciences upon man's present apprehension of himself and his powers, rather than the ways in which science has merely implemented man." A number of well-known scientific men have been invited to discuss these addresses, and it is expected that about two hundred and fifty registered members of the conference will take part.

The general public will be invited to attend and hear the program, part of which will be broadcast. The symposium is being preceded by six radio colloquies, by members of the faculty of Union College, one each Saturday afternoon. These include a round table discussion held by the department of mathematics on February 8, and by the department of physics on February 15. On succeeding Saturdays round table discussions will be conducted in chemistry, geology, psychology and biology.

NOMINATIONS OF OFFICERS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

THE National Nominating Committee of the American Institute of Electrical Engineers, consisting of members from various parts of the country, has nominated the following candidates for the offices becoming vacant August 1. These official candidates, together with any independent nominees that may be proposed later in the manner specified by the Constitution and By-laws, will be voted upon by the membership at the coming election this spring.

For President: DAVID C. PRINCE, manager, Commercial Engineering Department, General Electric Company, Schenectady.

For Vice-Presidents: (Middle Eastern District) N. S. HIBSHMAN, associate professor of electrical engineering, Lehigh University.

(Southern District) J. ELMER HOUSLEY, superintendent of power, Aluminum Company of America, Alcoa, Tenn.

(North Central District) ARTHUR L. JONES, commercial vice-president and district manager, General Electric Company, Denver.

(Pacific District) WALTER C. SMITH, engineer of Pacific District, General Electric Company, San Francisco.

(Canada District) C. A. PRICE, chief engineer, Canadian Westinghouse Company, Ltd., Hamilton, Ontario, Canada.

For Directors: LESTER R. GAMBLE, electrical engineer, Washington Water Power Company, Spokane; T. G. LECLAIR, supervising development engineer, Commonwealth Edison Company, Chicago; FRED R. MAXWELL, JR., professor of electrical engineering, University of Alabama.

For National Treasurer: W. I. SLICHTER, professor and head of Department of Electrical Engineering, Columbia University.

APPOINTMENT OF DR. JAMES BRYANT CONANT AS HEAD OF SCIENTIFIC MISSION

DR. JAMES BRYANT CONANT, president of Harvard University, formerly Sheldon Emory professor of organic chemistry, has been appointed by President Roosevelt head of a mission charged with reporting directly to Washington "recent scientific information of importance to national defense." Dr. Conant sailed on February 15 for Lisbon on the American Export liner *Excalibur*. He was accompanied by Frederick L. Hovde, since 1936 assistant to the president of the University of Rochester, previously assistant professor of chemistry at the University of Minnesota, and by Carroll L. Wilson, special liaison officer of the National Defense Research Committee, previously assistant to Dr. Karl T. Compton, president of the Mas-

sachusetts Institute of Technology. Dr. Conant will remain in England for about a month and Mr. Hovde will stay there as permanent secretary.

The White House statement said that "first-hand observation of recent English scientific research and experience is important for the prosecution of America's program of research on problems of national defense.

"As such collaboration proceeds, other American scientists will be sent from time to time to study different phases of British scientific research of interest to the United States and exchange information through the medium of the National Defense Research Committee.

"It is the duty of the National Defense Research Committee to 'correlate and support scientific research on the mechanics and devices of warfare.' Dr. Conant's mission is essential to that function."

Besides Dr. Conant, other technical members of the committee are Dr. Vannevar Bush, head of the Carnegie Institution of Washington and of the National Advisory Committee for Aeronautics, who is chairman; Dr. Richard C. Tolman, California Institute of Technology; Dr. Karl T. Compton, president of the Massachusetts Institute of Technology; Conway P. Coe, United States Commissioner of Patents; Dr. Lyman J. Briggs, director of the National Bureau of Standards, and Dr. F. B. Jewett, president of the National Academy of Sciences.

RECENT DEATHS

DR. FRANK CRAIG JORDAN, professor of astronomy and director of the Allegheny Observatory, Pittsburgh, and Mrs. Jordan, died in a fire that occurred in their home at Pittsburgh on February 15. Dr. Jordan was seventy-five years old.

DR. WILLIAM WEBBER FORD, professor emeritus of bacteriology at the Johns Hopkins University, died on February 10 at the age of sixty-nine years.

SAMUEL HENSHAW, director emeritus of the Harvard Museum and of the Harvard Museum of Comparative Zoology, died on February 5 at the age of eighty-nine years. He retired in 1927 after serving as director of the Harvard Museum since 1918 and of the Museum of Comparative Zoology since 1912.

DR. DE LISLE STEWART, lecturer in astronomy at the University of Cincinnati, president of the Cincinnati Astronomical Society, died on February 8 in his seventy-first year.

DR. WENDELL T. BUSH, professor emeritus of philosophy at Columbia University, who was connected with the university from 1905 until his retirement in 1938, died on February 12 at the age of seventy-four years.

DEATHS in England include: Dr. William Bulloch, professor emeritus of bacteriology at the University of London, a fellow of the Royal Society of Medicine, on February 11, in his seventy-third year; Frank Thomas Paul, professor emeritus of surgery at the University of Liverpool, on January 17, at the age of eighty-nine years; Dr. Miles Walker, for twenty years professor of electrical engineering on the faculty of technology of the University of Manchester, on January 22, at the age of seventy-three years, and Dr. D. Robertson, since 1902 professor of electrical engineering at the University of Bristol, on January 8, at the age of sixty-five years.

SCIENTIFIC NOTES AND NEWS

AT the Founders Day exercises of Temple University, Philadelphia, on February 14, an honorary degree was conferred on Dr. Henry A. Pilsbry, special curator of the department of Mollusca of the Philadelphia Academy of Natural Sciences.

DR. ALDEN H. MILLER, who was recently appointed to succeed the late Dr. Joseph Grinnell as director of the Museum of Vertebrate Zoology of the University of California at Berkeley, has been elected a corresponding member of the Zoological Society of London.

DR. PAUL DYER MERICA, assistant to the president of the International Nickel Company, received at the British Embassy, on February 10, a platinum medal awarded by the Council of the Institute of Metals of Great Britain in recognition of "distinguished services to nonferrous metallurgy." Dr. Merica originated the "precipitation theory" of hardening, developed in connection with research on aluminum alloys.

THE Marconi Memorial Medals of the Veteran Wireless Association were awarded at the sixteenth annual dinner in New York on February 10 to Major General J. O. Mauborgne, chief signal officer of the United States Army, and Admiral Leigh Noyes, director of naval communications. A plaque was presented to James L. Fly, chairman of the Federal Communications Commission, and Marconi Memorial Scrolls of Honor were awarded Richard Nebell, radio aid to the signal officer of the Second Corps Area, and David Karp, for meritorious service in the world war. G. H. Clark, former president of the association, received the Marconi Medal of History.

THE Association of American Geographers at its annual meeting, which was held at the State University of Louisiana, elected Dr. Griffith Taylor, of the University of Toronto, *president;* and Dr. Forrest Shreve, of the Desert Laboratory, Tucson, Ariz., *vice-president*.

DR. LAWRENCE CRAWFORD, professor of pure mathematics at the South African College at Cape Town, has been elected president of the Royal Society of South Africa.

SIR GEORGE SIMPSON has been reelected president of the Royal Meteorological Society, London.

SIR OWEN WILLANS RICHARDSON, Yarrow research professor of the Royal Society, has been elected to an honorary fellowship in Trinity College, Cambridge. Sir Owen was professor of physics at Princeton University from 1906 to 1914.

PROFESSOR W. M. SMART, regius professor of astronomy in the University of Glasgow, has been appointed Halley Lecturer at Oxford for 1941.

SURGEON REAR-ADMIRAL G. GORDON-TAYLOR has been appointed Bradshaw Lecturer of the Royal College of Surgeons, London, and Dr. L. R. Braithwaite has been appointed Moynihan Lecturer.

DR. GEORGE W. HOLMES, clinical professor of roentgenology at the Harvard Medical School and chief roentgenologist at the Massachusetts General Hospital, Boston, will become professor emeritus on September 1.

PROFESSOR CLARENCE T. JOHNSTON, chairman of the department of geodesy and surveying of the University of Michigan, has retired from active service.

PROFESSOR IDA OGILVIE, chairman of the department of geology of Barnard College, will retire at the end of this semester after thirty-eight years' service at the college.

DR. HENRY FRIEZE VAUGHAN, for twenty-two years Commissioner of Health of the City of Detroit, a son of the late Dr. Victor C. Vaughan, who was dean of the School of Medicine of the University of Michigan from 1891 to 1921, will join the faculty of the new School of Public Health of the University of Michigan as professor of public health. With Dr. John Sundwall, director of the Division of Hygiene and Public Health, and members of his staff, Dr. Vaughan will work out the reorganization of the present curriculum for application in the school. They will also supervise the planning of the new building.

DR. ALFRED BLALOCK, professor of surgery at Vanderbilt University, with which he has been connected for sixteen years, has been appointed professor of surgery at the Johns Hopkins University, the appointment to take effect on July 1. He is the third incumbent of the chair, the first being the late Dr. William S. Halsted. His immediate predecessor was Dr. Dean DeWitt Lewis, who retired in 1939.

DR. CHESTER ARTHUR STEWART, clinical professor of pediatrics at the School of Medicine of the University of Minnesota, an authority on child tuberculosis, has been appointed professor and head of the department of pediatrics in the School of Medicine of the Louisiana State University, the appointment to become effective on July 1.

PROFESSOR DAVID L. ARM, of Purdue University, has been appointed professor of mechanical engineering at the Iowa State College.

DR. S. A. GOUDSMIT, professor of physics at the University of Michigan, has leave of absence for the second semester to conduct graduate courses at Harvard University.

DR. FRITZ LEVY, director of laboratories of the Davis Memorial Hospital, The Golden Clinic, Elkins, W. Va., has received a grant from the Committee on Scientific Research of the American Medical Association, in aid of his work on megacariocytes and abnormalities of cell division.

DR. S. ANSBACHER, associate in charge of vitamin research in the Division of Experimental Medicine at the Squibbs Institute of Medical Research at New Brunswick, N. J., has become research director of the International Vitamin Corporation, New York City.

DR. J. E. GORDON, professor of preventive medicine and epidemiology at Harvard University, now head of the hospital recently established in England under the auspices of the university and the American Red Cross, has been appointed United States liaison officer with the British Ministry of Health on the invitation of Malcolm MacDonald, minister of health.

CHARLES P. TAFT, of Cincinnati, has been appointed by Paul V. McNutt, security administrator, assistant coordinator of all health, welfare, nutrition and recreation activities affecting national defense.

THE *Journal* of the American Medical Association states that Professor Chaim Weizman, president of the Jewish Agency and director of the Daniel Sieff Research Institute at Rehovot, Palestine, has been appointed a scientific adviser to the British government, and that a permanent laboratory has been put at his disposal. In 1914 Professor Weizman was lecturer in chemistry at the University of Manchester.

DR. P. W. BRIDGMAN, Hollis professor of mathematics and natural philosophy at Harvard University, spoke on February 20 at a joint meeting of the Washington Academy of Sciences and the Philosophical Society of Washington. His address was entitled "The Changing Position of Thermodynamics."

DR. PETER K. OLITSKY, member of the Rockefeller Institute for Medical Research, lectured in the auditorium of the Children's Hospital Research Foundation, Cincinnati, on February 12, under the auspices

of the local chapter of Sigma Xi, the Rachford Memorial Lectureship Fund and the Children's Hospital Research Foundation. The title of his address was "Hans Zinsser and His Studies on Typhus Fever."

DR. CYRUS C. STURGIS, professor of internal medicine at the Medical School of the University of Michigan, delivered the Henry Sewall Memorial Lectures at the School of Medicine of the University of Colorado at Denver on February 11 and 13. His subjects were "The Therapeutic Value of Blood and Blood Substitutes" and "The Hemorrhagic Diseases."

THE sixth annual Hughlings Jackson lecture of the Montreal Neurological Institute was delivered on January 29 by Dr. S. W. Ranson, whose subject was "Some Experimental Studies on the Corpus Striatum."

CAPTAIN NICHOLAS HUNTER HECK, chief of the Division of Geomagnetism and Seismology of the U. S. Coast and Geodetic Survey, delivered a Sigma Xi lecture entitled "Some Recent Developments in Earthquake Study" at the Utah State Agricultural College on January 24. While in Logan, he inspected the Oldham Seismograph Station which is conducted by the department of geology of the college with the cooperation of the Coast and Geodetic Survey.

THE fifty-seventh meeting of the American Association of Anatomists, by invitation of the University of Chicago, will be held at the university on April 9, 10 and 11. The chairman of the Local Committee is Dr. Charles H. Swift.

THE National Foundation for Infantile Paralysis will hold a semi-annual meeting of its Medical Committees on May 15. At this time applications for grants for the study of the cause, prevention and treatment of this disease will be considered. Applications to be considered at the semi-annual meeting must be received at the office of the Foundation, 120 Broadway, New York City, on or before March 15.

THE Chicago Academy of Sciences, in cooperation with the Wm. I. Lyon Bird-banding Council, is sponsoring a conference at the academy on Saturday, March 15. Papers will be presented on various phases of ornithological work with special reference to bird-banding. In addition a photographic exhibit and various types of traps used in bird-banding will be on display. There will be a dinner in the evening followed by an address by Bob Becker, of *The Chicago Tribune*.

WORD has been received by Dr. Leonard Carmichael, president of Tufts College, that a new "China Institute of Physiology and Psychology" has been established at Chungking. The well-known Chinese psychologist and experimental embryologist, Dr. Zing Yang Kuo, has been named director of the institute.

AN institute for endocrinology has been founded at São Paulo, Brazil, consisting of three departments devoted, respectively, to experimental endocrinology, human endocrinology and a laboratory for testing opotherapeutic products.

A NEUROPHYSIOLOGICAL section has recently been added to the Carolinian Medical Institute in Stockholm, principal medical school of Sweden. It is under the direction of Professor Ragnar Granit, an authority on electrophysiology. Professor Granit has taken with him from Helsingfors electrical and optical precision instruments, partly acquired or designed with funds contributed by the Rockefeller Foundation.

BEQUESTS amounting to a million and a half dollars are made in the will of Caroline White, of New York City, to churches, institutions and individuals. Among these six New York hospitals receive each the sum of $100,000.

ACCORDING to the will of Mrs. Charles Henry Thomas, the bulk of her estate of $1,359,654 will go eventually to Radcliffe College, Yale University and the Massachusetts Historical Society, Boston. Radcliffe College will receive the residuary estate, valued at $548,397, plus the remainder of a $500,000 life trust fund, and the remainder of a life trust fund of $35,000. It is requested that the money be used to establish scholarships or fellowships in music. Yale University will receive an outright gift of $50,000, plus the remainder of a $50,000 trust fund.

THE Journal of the American Dental Association announces a gift of $50,000 to Northwestern University for the study and treatment of cleft palate, harelip and similar facial and oral abnormalities. The work will be under the direction of Dr. Frederick W. Merrifield of the medical and dental faculties.

THE Blandy Experimental Farm of the University of Virginia at Boyce, has been granted $40,000 by the General Education Board for additions to the present brick building and for equipment to provide adequate laboratories and living quarters and $5,000 for the construction of a greenhouse.

AT a recent meeting of the Royal College of Surgeons, London, a resolution of thanks was passed for the gift of $7,500 from the Rockefeller Foundation for evacuating the library and research laboratories.

PROFESSOR JAMES R. JEWETT, of Cambridge, Massachusetts, has recently presented to the Arnold Arboretum the sum of $5,000 to be known as the James R. Jewett fund. Under the terms of gift the income may be used for first and second prizes to be known as the James R. Jewett and the Vieno T. Johnson prizes. These prizes are, for the present, to be awarded to individuals who have made significant con-

tributions to the improvement of our native beach plum (*Prunus maritima*), or who, through the development of beach plum products, may have made contributions of social significance. It is planned to make the first awards in August, 1941. Those interested may secure additional information by communicating with the director, Arnold Arboretum, Jamaica Plain, Mass. The awards will be made by a special committee to be appointed later in the year.

UNDER the will of the late Sir Henry Head, the Royal Society is named as residuary legatee of his estate, which is valued at £144,588. It is provided that the money "shall be applied for the purpose of the advancement in England of the science of medicine in the widest sense." The will suggests that the fund, or part of it, might be used to establish professorships or similar posts or research scholarships in some branch of medical science.

THE J. T. Baker Chemical Company has announced that its Eastern fellowship for research in analytical chemistry is open for 1941–42. This fellowship is to encourage fundamental research in analytical chemistry. The recipient will receive $1,000 annually and will be expected to devote at least nine months to research in an institution conferring the Ph.D. or Sc.D. degree in chemistry in one of the New England states, New York, New Jersey, Pennsylvania, Delaware, Maryland or Virginia. The fellowship is awarded by a committee consisting of N. H. Furman, chairman, Princeton University; J. H. Yoe, secretary, University of Virginia; G. P. Baxter, Harvard University; H. A. Fales, Columbia University, and C. W. Mason, Cornell University.

WITH the approval of the United States Office of Education, Cornell University has opened a course in Mechanics and Elementary Design of Aircraft Structures in Buffalo in cooperation with the University of Buffalo. This course, supplementing one established on October 7 without Federal aid, is also given at the request of the Curtiss-Wright and Bell Aircraft Companies. Federal approval of the new course means that there will be no tuition charge, since the expenses will be assumed by the Government under the recent act appropriating funds for Engineering Training for National Defense. The course is designed to meet an urgent national defense need for men trained for junior engineering positions in the airplane industry. High-school graduates who have completed trigonometry and mechanical drawing are eligible to enrol. The course will comprise an intensive study of statics, mechanics of materials and elementary design, with special attention to design of details of aircraft structures. More than three hundred students are enrolled, according to Dr. Arthur S. Adams, professor of mechanics in the College of Engineering at Cornell and director of the Buffalo courses.

A NEW division of chemotherapy has been formed in the National Institute of Health of the U. S. Public Health Service to be under the direction of Dr. William H. Sebrell, Jr. The *Journal* of the American Medical Association states that the new unit will be concerned with research on sulfanilamide products and with new synthetic drugs with antimalarial properties to make the United States independent from the Dutch East Indies supply of quinine, new synthetic drugs to supplement the supplies of opiates and studies relating to aging and nutrition. The chemotherapy division will have quarters at the National Institute of Health in Bethesda, Md., in a laboratory building now being equipped. The building will also be the headquarters of the divisions of chemistry and zoology.

DISCUSSION

AN IMPORTANT FACTOR IN EVOLUTION

SEWELL WRIGHT in Julian Huxley's "The New Systematics" (Oxford, 1940, p. 174) says: "The one *systematic* effect of mutation seems to be a tendency towards degeneration (as may be seen from a casual survey of the effects of most of the *Drosophila* mutations)." I believe that this observation reveals an evolutionary factor of primary importance.

It has long been appreciated that most of the *Drosophila* mutations are degenerative. We have been told that such mutations are rapidly eliminated under natural conditions by selection, which waits upon the occasional favorable mutation to effect evolutionary change. On the other hand, taxonomists have long realized that an adaptive significance can not be attributed to most of the known taxonomic differences distinguishing species and genera. Taxonomists and comparative anatomists have likewise appreciated the great extent to which the evolutionary changes in related groups of organisms involve the simplification, fusion or loss of old parts, and how rarely new parts or more complicated parts come in. It is now possible to point out the connection between these several sets of facts.

As a result of the principle that the loss of genes from the chromosomal mechanism is more frequent than their addition, it follows that whenever evolutionary change is unrestricted by natural selection, a degeneration of structure is likely to result.

Not all degenerative change is, of course, non-adaptive. Frequently, structures of adaptive significance arise in this way, as when the anterior segments of the polychaete fused to form the arthropod head.

Under normal conditions natural selection confines degenerative changes to minor body features, as details of sculpture or color pattern, but under abnormal conditions more profound changes are likely to occur. Cave conditions, as Wright himself mentions, are a case in point. In a normal environment selection tends to keep up to par the light receptive and light protective mechanisms, *i.e.*, the visual and pigmentation systems. In caves, on the other hand, such factors are relaxed so that these systems degenerate. Tactile organs, however, such an antennae and long body-setae, are encouraged, at least in the ground beetles, so that these structures thrive and are maintained in a high degree of development. Similarly explained are the degenerative changes that occur in sedentary and parasitic animals in general and the occasional loss of wings by birds and insects in certain insular environments. Darwin took account of the more extreme of these degenerative changes under the heading of the effect of disuse. But we have long realized that "disuse" in itself has no influence on the hereditary mechanism. It is merely evidence that the parts in question are no longer useful to the organism. They are, accordingly, no longer maintained by natural selection and so are subject to degenerative modification.

The picture that we get of evolution is somewhat as follows: Mutations constitute the basic material. The upbuilding of the complexity of organic function and structure and the thoroughgoing adaptation that all organisms exhibit are the product of natural selection. The detailed specific and generic differences are largely the result of differential mutational effects in partially or completely isolated populations. Selection effects a continuous influence, keeping the organism and its parts up to a certain par and preserving characters of adaptive significance as they occur. There are, however, always important portions of every organism over which selection effects no influence, and these are subject to degenerative changes in accordance with the principle that the loss of genes is more frequent than their gain. Still unexplained, perhaps, are certain so-called orthogenetic changes, like the suture system of ammonites, but the principle of degenerative evolution in the absence of continued selective control seems to be an important factor in understanding plant and animal diversity.

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LINNÆUS ON THE NATURAL HISTORY OF MAN

DR. HRDLIČKA'S communication in SCIENCE for December 27, 1940, containing a passage from a letter written by Linnæus in 1747 on the similarity of man and the apes reminded me of a note I made a few years ago in reading Linnæus' "Tour in Lapland." In his journal of this tour, under date of July 11, 1732, Linnæus wrote, "If we contemplate the characters of our teeth, hands, fingers, and toes, it is impossible not to perceive how very nearly we are related to Baboons and Monkeys, the wild men of the woods." Thus we find Linnæus at the age of twenty-five and nearly fifteen years before his letter of February 14, 1747, to Gmelin, perceiving a close relationship between man and the monkeys. My quotation is from page 331 of the first volume of *"Lachesis Lapponica,* or A Tour in Lapland, Now First Published from the Original Manuscript Journal of the Celebrated Linnæus; by James Edward Smith, M.D., F.R.S., President of the Linnæan Society" (London, 1811). The editor's preface says that the original was written in Swedish intermixed with Latin, that a literal translation of the Swedish was made by "Mr. Charles Troilius, a young gentleman in the mercantile line, resident in London," and that he himself put the matter into its final English form. Though we have no assurance of the absolute literalness of the translation of the particular sentence I have quoted, there seems to be no reason to doubt that it expresses fairly what Linnæus wrote in his journal.

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THE SOYBEAN IN CHINA

IN your issue of January 24, 1941 (Vol. 93, No. 2404), in a report on "the soybean crop in the United States" by A. W. von Struve, I find the assertion: "The first record of the plant is in the writings of Emperor Shang Nung of China in 2838 B.C."

Elmer D. Merrill and Egbert H. Walker ("Bibliography of Eastern Asiatic Botany," 1938, p. 556) call Shên-nung (not Shang Nung) "an early mythical Chinese ruler," H. A. Giles ("Chinese Biographical Dictionary," 1898, No. 1,695) "a legendary emperor," and B. Laufer, more correctly ("Beginnings of Porcelain," 1917, p. 160) "the culture hero, who, as implied by his name, 'Divine Husbandman,' was regarded as the father of agriculture and discoverer of the healing properties of plants."

No one knows just when the herbal attributed to him was compiled, but it probably was not before the Christian era. Philip K. Reynolds and Mrs. C. Y. Fang (*Harvard Journal of Asiatic Studies,* V, 2, June,

1940, p. 171) say: "most likely in the first or second century."

As to the soybean, no indication of it has been found, I believe, in neolithic or early bronze age sites. But it seems to appear in literature of the first millennium B.C. (*cf.* Creel, "Birth of China," p. 326).

References to early dates in Chinese history are likely to be unreliable for any century prior to the eighth before our era.

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OUR SCIENCE MEETINGS AGAIN

IT is getting to be an annual custom to ask, "What *is* the matter with our scientific meetings?" Dr. Merrill asked it quite some years ago on his return to America after a long sojourn abroad. As I remember, he looked forward with enthusiasm to the meetings of the A. A. A. S., his first in many years. And when they convened he wandered from room to room hoping, ever hoping, that in the next room he would find a speaker who could hold his attention. But his wanderings availed him nothing. He made the suggestion that our A. A. A. S. gatherings should not be given over to youngsters who are making their first public appearance. This is true, but it would be just as disastrous if they were given over to the older men. What is needed is a better balance between the stability of older men and the daring enthusiasm of youth. I attempted this in arranging a symposium at which five out of nine speakers were under fifty and two under thirty years of age.

IN a recent issue of SCIENCE (93: 19, 1941), Dr. Francis H. Allen suggests that speakers should improve their delivery. This is important and would help, but it is, so it seems to me, a rather futile attempt at improvement because poor delivery is mostly due to personal traits which are very difficult to correct. To be sure, as Dr. Allen says, we can all learn to hold our heads up and direct our voices toward the back of the room, but to correct a weak voice, which mumbles along in a monotone can be done only by starting when the otherwise able scientist is six years old. If I may suggest several rules for "the acquirement of a clear and common-sense manner of communicating information and ideas to an intelligent audience," then they are the following.

First, foremost and irrevocable should be the rule forbidding the reading of a paper. This will go hard with some, but if a scientist does not know his subject well enough to present it extemporaneously then he should wait until he does. There is usually opportunity for practice on the students and at the home seminar, before the big meetings come. If this rule seems harsh, then attend the next symposium at which some six or eight papers are read, and you will approve the rule wholeheartedly. All other rules are subordinate to this one.

Second in importance is the elimination of historical introductions and lengthy summaries. The latter is as necessary and as effective as a twice-told joke. All mathematical tables and curves should be reduced to a minimum. A column of numbers may help much toward understanding what took place in an experiment, but five tables of five columns each shown one after another leave the audience utterly bewildered. Curves portray a situation with graphic clarity and are always desirable when there is time to study them. Three or four are about all that a speaker can interpret and an audience digest in a half-hour talk, yet a dozen curves at the rate of two a minute is not uncommon at our meetings. These are rules which all can follow and all will be grateful for when they are observed by others.

The greatest lack in our science meetings is discussion. An abstract is printed, the audience has read it, and the speaker repeats it, twice if he gives a summary. It is the awful routine of the papers which is so deadly. One speaker after another, and no comments. If each group could be organized in advance and discussion arranged for, this more than any other change, would make for an interesting meeting.

And all after-dinner talks should be limited to twelve minutes!

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THE PRESENTATION OF SCIENTIFIC PAPERS

THOSE who read F. H. Allen's suggestions about improving the presentation of scientific papers (SCIENCE, 93: 19, 1941) undoubtedly will be interested to learn that the Western Section of the American Society of Plant Physiologists has already taken steps to make scientific meetings more enjoyable by improving the presentation of papers. This question was brought up at the annual meeting during the summer of 1939. The following spring when the call for papers (for the Seattle meeting) was issued, I included the suggestion that prospective speakers *refrain from reading prepared manuscripts, but use a few notes instead.* This suggestion was followed up almost without exception and resulted in better contact between audience and speaker. Because the meetings were followed by an excursion (to Friday Harbor) which lasted a whole day, I had an opportunity to talk to people and inquire about their reaction to the meetings. Many stated that they did not experience the fatigue which is so common after meetings. Although this might have been partly due to the invigorating atmosphere of the Pacific Northwest, I am convinced that the informal

way of presenting papers must have been easier on both the audience and the speakers. The reasons for this are: (1) Most people are able to talk understandably; few are able to read a paper understandably. (2) While talking, the speaker has to formulate his thoughts, which gives the audience a chance to catch up with him. When reading, however, the speaker does not have to formulate his thoughts, and the audience usually does not have enough time to resynthesize words into thoughts.

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SCIENTIFIC BOOKS

THE DESIGN OF HIGH PRESSURE PLANT

The Design of High Pressure Plant and the Properties of Fluids at High Pressures. By DUDLEY M. NEWITT, assistant professor of chemical technology in the Imperial College of Science and Technology. viii + 491 pages, 24.3 × 15.5 cm. 165 figures in the text and 4 plates, with 86 tables in the text and 18 pages of tabular material in the three appendices. Published by the Clarendon Press, Oxford, England, and obtainable in this country from the Oxford University Press, 114 Fifth Ave., New York City, for $10.00.

THE purpose and limitations of this book are doubtless more evident from the title to the English reader than to the American, to whom the use of "plant" in its English technical sense is somewhat unfamiliar. The author attempts no explicit statement of his purpose and limitations, but an examination of the contents shows that its main purpose is to serve as a working manual and complete reference book for the industrial chemist primarily interested in gaseous reactions or other industrial processes such as liquefaction in the range up to 1,000 atmospheres; there is, however, considerable material dealing with liquids up to 12,000. Within its field the book should prove of much usefulness, since the material has never been collected into one place, and much of it is otherwise available only in the original papers.

Consistently with its purpose the book begins with a discussion of the mechanical properties of the materials, mostly steels, from which the pressure apparatus is to be constructed, and the effect of both high and low temperatures on these properties. There follows a chapter, primarily of interest to British readers, on the technical and legal requirements on the cylinders used for the transport of compressed gases. The next chapter discusses the design according to elasticity theory of cylinders for withstanding internal pressure, including built-up and auto-frettaged cylinders; much of this material is valuable and not otherwise easily available. Chapters follow on details of packing, fittings and measurement of pressure. The rest of the book, 360 pages, is devoted to the properties of fluids under pressure and contains much which will interest the physicist not concerned in the narrow technical sense with this field; it is especially to be recommended as a source for numerical data in a course on thermodynamics. There is a full discussion of critical phenomena and the relations between liquid and vapor phases with a discussion of various equations of state. Mixtures of different gases are then discussed. There follow two long chapters on the thermodynamics of gases. Coexisting liquid and vapor phases of binary and ternary mixtures are then treated. M. Ruhemann contributes an interesting chapter on the liquefaction of gases, in which the emphasis is more on the fundamental thermodynamics than in the rest of the book. Chapters follow on the effect of pressure on the solubility, viscosity, dielectric constant and refractivity of gases. The last 50 pages are devoted to proper liquids, in particular, the effect of pressure on viscosity and refractivity, and the pressure-volume-temperature relationships.

As a whole the book should be of much utility, and doubtless every technical worker will insist on having it where it is available, but in using it allowance will have to be continually made for possible lack of completeness. The scheme by which the material has been selected is not at all clear, and there are omissions for which the explanation is not obvious. The following may be mentioned which struck me because I am personally concerned. There is no mention of any of my own work later than 1929 nor of my book on high pressure of 1931. The result is that there is no mention of determinations of the pressure-volume-temperature relations of some fifty liquids, although my earlier work in 1912 on fourteen liquids is described in some detail. It seems that some mention should have been made of the work of Benedicts on the pressure-volume-temperature relations of gaseous nitrogen over a range much wider than that of other observers, and of the work of Birch on the critical data for mercury at the highest temperature and pressure at which critical points have been measured.

P. W. BRIDGMAN

ABSTRACT ALGEBRA

An Introduction to Abstract Algebra. By CYRUS C. MACDUFFEE. 303 pp. John Wiley and Sons. $4.00.

A CONSPICUOUS mathematical development of the last two decades has been the growth of algebra as a unified science, fruitful in applications to modern

physics and chemistry, as well as to other branches of mathematics. The general methods and results of the newer algebra were first made available to professional mathematicians as a whole by van der Waerden's now classic "Moderne Algebra." However, besides being written in a foreign language, this book was far too advanced and compendious to be a suitable text for a standard graduate course in this country.

MacDuffee's new volume is the second noteworthy attempt to provide such a text, the first being Albert's "Modern Higher Algebra." Although it covers much less ground than Albert's book and contains no original material, MacDuffee's book, as its title suggests, affords an easier introduction to abstract algebra than Albert's. By emphasizing the most basic theorems and making no attempt at completeness, MacDuffee drives home the fundamental ideas of modern algebra. And by illustrating each definition with carefully chosen examples, he gives "concrete" significance to them—a difficult feat in so-called "abstract" algebra.

The book is not designed for purposes of reference or for use in advanced seminars. However, considerable ground is covered in the theory of algebraic numbers, including the Kronecker program of developing the real and complex number systems from the rational integers, finite fields, valuations and p-adic numbers. This emphasis gives the book an arithmetic bias, especially in the first 200 pages. The last third is devoted to matrices, concerning which the author has already written a standard reference work, and to linear associative algebras. Like van der Waerden and Albert, the author has in effect presupposed "the traditional" course in the theory of equations, and has not dealt with applications outside the domain of pure algebra.

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SPECIAL ARTICLES

ON LUBIMENKO EXTRACTS OF CHLOROPHYLL-PROTEIN

IT has often been suggested that chlorophyll in the living plant is the prosthetic group of a conjugated protein and that when chlorophyll is extracted with an organic solvent it is separated from the protein to which it was originally attached. When, however, one avoids organic solvents and extracts ground leaves with water or salt solution, one usually obtains not a water-soluble chlorophyll-protein but a suspension of chloroplasts from which the chloroplasts are separated by filtration or centrifugation. A little water-soluble chlorophyll can be extracted from chloroplasts by bile salts[1] and much more by the modern synthetic detergents.[2] But it is not safe to use these reagents for the extraction of native, unmodified protein because they are known to denature proteins.[2]

Probably the most interesting and promising observations concerning water-soluble chlorophyll-protein compounds have been made by Lubimenko,[3] whose paper has never received the attention it merits. In the course of a very extensive survey of the chemical natural history of chloroplasts and their pigments, Lubimenko found that a few plants, in particular the lilies, *Aspidistra elatior* and *Funkia,* yielded on extraction of their leaves with water at his unspecified Russian room temperature not suspensions of chloroplasts but opalescent solutions from which the green pigment was not removed by filtration. Lubimenko gave good evidence that the chlorophyll in his opalescent extracts was bound to protein. His evidence was similar to the evidence that visual purple in the eye is bound to protein.[4]

Following Lubimenko, I ground leaves of a species of *Funkia* found in a local garden with sand, extracted the ground leaves with dilute sodium chloride, removed insoluble material by centrifugation with a Swedish angle centrifuge or filtration through Hyflo Super-Cel (Johns-Manville), and obtained a green opalescent solution in which no particles could be seen with an oil-immersion lens. The chlorophyll in the extract was much more unstable than I had expected from reading Lubimenko's paper. At 20–25° C. an appreciable amount of the chlorophyll in the extract went over into an insoluble form within an hour. This process was greatly speeded up by raising the temperature to 37° C., greatly slowed down by lowering the temperature to 0° C. All operations with Lubimenko extracts should, therefore, be carried out in the cold. Freezing, however, should be avoided, since it makes the chlorophyll-protein insoluble.

I have investigated many plants available in the neighborhood and in the Institute greenhouses and found that opalescent Lubimenko extracts can be prepared from a number of legumes, in particular the Early Golden Cluster variety of bean, *Phaseolus vulgaris* L., and the Black variety of cow pea, *Vigna sinensis* Endl., as well as from the lilies used by Lubimenko. These legumes have the great advantage that they can be grown readily and rapidly in the greenhouse throughout the year from seeds which are al-

[1] E. Smith, SCIENCE, 88: 170, 1938.
[2] M. L. Anson, SCIENCE, 90: 142, 1939: *Jour. Gen. Physiol.*, 23: 239, 1939.
[3] V. N. Lubimenko, *Rev. Gen. Bot.*, 39: 619, 1927.
[4] W. Kühne, in L. Hermann, "Handbuch der Physiologie," pt. 2, Leipzig; F. C. W. Vogel, 3: 264, 1879; G. Wald, *Jour. Gen. Physiol.*, 19: 351, 1935.

ways available in quantity. It should be pointed out that although the legumes used are unusual in yielding opalescent solutions, they resemble other plants in having chloroplasts about the size of red blood corpuscles.

The chlorophyll in the Lubimenko extracts I prepared was partially sedimented on long centrifugation in the cold in a Swedish angle centrifuge operating at top speed, and almost completely sedimented on 4 minutes centrifugation in the cold in an ultra-centrifuge operating at 20,000 r.p.m. under conditions under which tobacco mosaic virus is not sedimented to any significant extent. Thus the green particles in Lubimenko extracts are much smaller than chloroplasts, but larger than tobacco mosaic virus molecules. The exact size of the green particles, the extent of their homogeneity in size, and their rôle in photosynthesis remain to be studied.

High molecular weight particles with some color have previously been obtained from cow pea and cucumber leaves.[5] Unlike the green particles in the present extracts they represented only a very small part of the chlorophyll of the leaves and they sedimented more slowly than tobacco mosaic virus. It is possible that the green particles previously observed were decomposition products of the particles as they exist in cold aqueous extracts. It is also possible that the larger particles in the present extracts represent aggregates of the original material.

In summary, it has been shown that aqueous extracts of chlorophyll-protein can be obtained from constantly and readily available plants, that these extracts can be stabilized by cold, and that the green particles in these extracts are larger than tobacco mosaic virus.

I am indebted to Dr. F. O. Holmes and Dr. W. C. Price for advice in botanical matters.

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THE H-ION CONCENTRATION AND THE ORIGIN OF THE HEART BEAT

IT has been suggested that the H-ion concentration is intimately concerned with the fundamental process originating the heart beat.[1] This suggestion was supported by experimental data which indicated that the H-ion concentration (within limits close to normal) was reciprocally related to the heart rate.[1, 2]

Recent investigators have stressed the necessity of distinguishing between the early, and often transient effects, and the later, more permanent effects which may be found in such studies.[3, 4] The results of these latter investigations throw some doubt on the value of the experimental procedures used to obtain evidence in support of the theory mentioned above.

The present experiments were carried out (*a*) on the excised sinus venosus of the frog heart suspended in aerated Ringer's solution and (*b*) on the right atrium of the guinea pig heart perfused by the method which I have recently described.[5]

In the experiments on the frog sinus venosus, the heart rate was counted at 10- to 15-minute intervals for at least 45 minutes or until the heart rate had become constant. The heart was then transferred to the experimental Ringer's solution and the heart rate was counted at the same intervals for 1 hour; the heart was then transferred back to normal Ringer's solution and the heart rate counted at intervals for at least 45 minutes. By observing the heart for such long periods of time, it is possible to distinguish between the transient and variable response which often occurs when the heart is transferred from one solution to another, even though the second solution may be of the same composition as the first, and the later, more permanent response.

The procedure with the guinea pig atrium was similar; however, 3 readings taken over a period of 30 minutes were found sufficient to determine the heart rate with each solution.

The results are given in the tables. The heart rate in each case is expressed as a decimal fraction of the heart rate in normal Ringer's solution. For the frog sinus venosus, the pH of the normal Ringer's solution was 7.7; for the guinea pig atrium, the pH was 7.52. The experimental solutions differ from the normal solution only in their pH values. The experimental values for the frog Ringer's solution were attained

[1] E. Cowles Andrus and Edward P. Carter, *Heart*, 11: 97, 1924.
[2] E. Cowles Andrus, *Jour. Physiol.*, 59: 361, 1924.
[3] J. J. Izquierdo, *Jour. Physiol.*, 68: 363, 1930.
[4] C. R. Spealman, *Am. Jour. Physiol.*, 124: 185, 1938.
[5] W. C. Price and W. G. Wyckoff, *Nature*, 141: 685, 1938; *Phytopath.*, 29: 83, 1939.

TABLE 1

RELATIVE HEART RATE VALUES OF THE FROG SINUS VENOSUS AT DIFFERENT pH'S*

pH													
2.9	3.5	4.8	5.7	6.1	6.8	7.1	7.4	7.7	7.8	8.6	8.8	9.4	9.7	10.2
S	S	0.98	0.93	1.10	1.06	0.89	1.00	1.00	0.96	0.89	0.95	1.00	S	S
S	0.95	1.00	0.90	1.00	0.98	1.02	1.06	1.00	–	1.02	1.00	–	S	S

* Each value represents a determination on one heart. The pH was adjusted with 0.1 N HCl or NaOH. 24 hearts were used in the experiments. S indicates the heart stopped beating in the experimental solution; – indicates no experiment was made.

by adding the necessary quantities of 0.1 N acid or base to normal Ringer's solution; those for the modified Locke's solution used with the guinea pig atrium, by adjusting the ration of acid phosphate to basic phosphate.

The rate of beat of the frog sinus venosus is not significantly affected when the pH is varied over a wide range. This is true whether highly dissociated acids or bases are present (Table 1) or whether weakly dissociated acids or bases are present (Table 2). Outside of a certain pH range, the sinus venosus usually stopped beating during the period of 1 hour in which it was in these solutions. The rate of beat of the guinea pig heart was not significantly affected by the pH in the region studied.

TABLE 2

RELATIVE HEART RATE VALUES OF THE FROG SINUS VENOSUS AT DIFFERENT pH'S*

pH				
6.2	6.7	7.2	7.7	8.2
1.00	0.94	0.97	1.00	1.03
1.00	1.05	0.95	1.00	1.05

* Each value represents a determination on one heart. The pH was adjusted with 0.1 N CH_3COOH or NH_4OH. 8 hearts were used in the experiments.

TABLE 3

RELATIVE HEART RATE VALUES OF THE GUINEA PIG RIGHT-ATRIUM AT DIFFERENT pH'S*

pH					
7.03	7.30	7.38	7.52	7.60	7.65
1.01	0.95	1.04	1.00	1.08	1.04
–	0.89	0.99	1.00	–	1.05

* Each value represents a determination on one heart. The proper pH's were obtained by adjusting the ratio of acid to basic phosphate. 4 hearts were used in the experiments. – indicates no experiment was made.

SUMMARY

The rate of the frog or guinea pig heart preparation was not significantly influenced by the H-ion concentration when this was kept within limits which allowed the heart to continue beating. The theory that the H-ion concentration is intimately concerned with the origination of the heart beat is not supported by the results of this investigation.

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SUSCEPTIBILITY TO DISEASE IN RELATION TO PLANT NUTRITION[1]

IF the severity of a plant disease is different under different fertilizer treatments or other environmental conditions affecting plant nutrition, it becomes important to know in what respects nutrition differs under the various conditions and in relation to the severity of the disease. The method of foliar diagnosis[2, 3] as the following example shows, enables the investigator to obtain this information.

In an experiment on greenhouse tomatoes grown on plots of which the cultural conditions, except the fertilizer treatment, were similar, certain plants on some of the plots began to show the characteristic symptoms of streak disease about 80 days after transplantation to the beds. This disease is known to be caused by a virus or a mixture of viruses; the symptoms are light yellow, elongated, irregular areas on young leaflets which later turn brown and die, and elongated brown lesions appearing longitudinally on stems. New growth may appear healthy from time to time during the season, and at other times may show the lesions just described. The latter characteristic serves to differentiate the disease symptoms from those of potash deficiency, in which new, healthy leaves do not develop after lesions appear.

Because the disease was wide-spread on certain plots and entirely absent from others, which were exposed equally to infection, but differed in manurial treatment, an examination was undertaken by the method of foliar diagnosis to compare the course of nutrition of low-yielding plants exhibiting symptoms of streak, grown on a plot fertilized with nitrogen only as commercial sodium nitrate, with that of high-yielding, healthy plants growing on a plot fertilized with rotted manure and complete commercial fertilizer.

In Fig. 1 are shown in trilinear coordinates the equilibrium between the dominant elements for the sixteenth leaf from the base as expressed by the composition of the *NPK-units*, which are derived by converting the percentage values of nitrogen, phosphoric acid and potash to milligram equivalents and finding the proportion which each of these bears to the milligram equivalent total. The samples were taken 85 days after the plants were placed in the beds.

The coordinates are shown for leaves from three types of plants on the plot having diseased plants, namely: plants showing no visible symptoms of disease at the time of sampling (II); plants showing slight symptoms (III); and plants severely diseased (IV). Comparison is made with morphologically homologous leaves from plants on a plot (No. 8L) on which no disease appeared (I). This latter received a complete fertilizer, together with well-rotted horse manure.

The coordinates (II, III and IV) of the leaves from the several types of plants from the plot No. 12L having diseased plants are displaced relative to those

[5] C. R. Spealman, *Proc. Soc. Exp. Biol. and Med.*, 45: 189, 1940.

[1] Authorized for publication as paper No. 978 of the Journal Series of the Pennsylvania Agricultural Experiment Station.

[2] Walter Thomas, *Plant Physiology*, 12: 571–600, 1937.

[3] Walter Thomas and Warren B. Mack, *Pa. Agr. Exp. Sta. Bull. No. 378*, pp. 1–33, 1939.

of the plants on plot No. 8L, none of which were diseased (I), further towards the summit apex (N = 100) and away from the right base apex (K_2O = 100), indicating a higher relative proportion of N and a lower proportion of K_2O in the composition of the respective *NPK-units*.

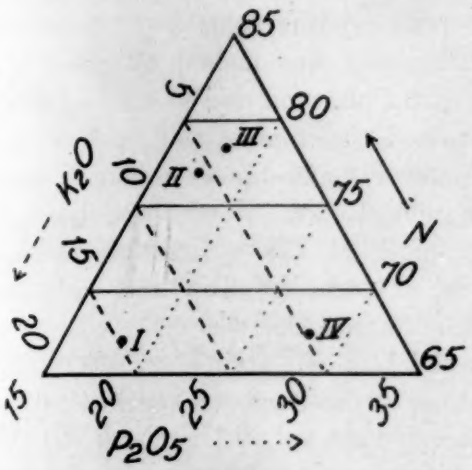

FIG. 1. Relative positions of coordinates of the 16th leaf from plants growing on plot No. 8L on which no disease appeared (I) and of those from plants on plot No. 12L showing no visible symptoms of disease (II), from plants showing slight infection (III), and from badly infected plants (IV). Only a portion of the triangle of which each side is 100 is shown.

The coordinate point (II) of leaves from plants on the diseased plot, which showed no visible symptoms of infection at this time, is displaced, however, further toward the left base apex (K_2O = 100) than that of leaves from the plants on this plot which showed visible manifestations of disease (III and IV).

The intensities of nutrition—the sum of the percentages of N, P_2O_5 and K_2O in the dried foliage—for the plants from the diseased plot were 4.86 (II), 4.43 (III) and 5.65 (IV), and for the healthy plants, 7.90.

The infection by the virus was associated with a type of nutrition having quantitatively a lower intensity with respect to the plastic elements, and qualitatively with a disequilibrium with respect to these elements, characterized predominantly by higher values for N and much lower values for K_2O in the composition of the *NPK-unit* of the susceptible compared with resistant plants.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

HISTOLOGICAL SECTIONING OF HARD TISSUES BY A NEW TECHNIQUE

VARIOUS concretions, bone and other hard or brittle tissues are not easily sectioned on the microtome. Since fluid methacrylates, the basis of "Lucite" and "Plexiglass," can be polymerized to solids *in situ*, it is possible to imbed such tissues in a solid medium firm enough to allow the grinding of very thin sections, using the well-known methods of petrography. We outline below some of the details of such a technique which we have used with good success.

The specimen, which we shall suppose to be about 1 cm on a side, is dehydrated, using the alcohol, acetone or dioxane technique, and is then cleared in xylol. Some care must be taken in the choice of a clearing agent, as some of these are not miscible with methyl methacrylate; while chloroform, for example, may decompose during the ensuing polymerization giving rise to bubbles. With dry substances, of course, these steps may be omitted. The specimen is next immersed in monomeric (unpolymerized) methyl methacrylate, a liquid having a sufficiently low viscosity to allow of complete penetration in twelve hours, using three changes. The monomer, as supplied commercially, contains a trace of hydroquinone in inhibit polymerization; and .05 per cent. benzoyl peroxide should be added to act as a catalyst for polymerization and consequent solidification upon heating. In the case of dry substances reduced pressures may advantageously be used to insure complete penetration of the liquid medium. A test-tube, of about 17 mm diameter, is prepared by polymerizing a 4 cm rod of solid methacrylate in its bottom, this to act as a temporary handle. The impregnated specimen is placed in the test-tube on top of this rod and covered with methacrylate, previously polymerized to the consistency of molasses.

The partial polymer is prepared by heating the catalyzed monomer to 80° C. until thick, about twenty minutes. The use of the partial polymer in this step helps to reduce the overall heat liberated by the reaction, while its presence catalyzes the polymerization of the monomer permeating the tissue. The syrup containing the specimen is now caused to become an integral extension of the solid rod by completing its polymerization in an oven at 40° C. The test-tube should be corked to prevent undue evaporation. This step requires about twenty-four hours, after which the test-tube is cracked away from the rod, which now contains the specimen firmly imbedded near one end. This rod is next cut through the specimen, normal to the rod axis, and the exposed surface, containing the tissue, ground flat and polished. For this grinding process successive grades of emery paper and mild polishing powders suffice.

If serial sections are desired the tissue-containing rod may be cut into wafers, each of which is polished

plane on one surface. In practice these wafers may be cut as thin as 0.2 mm, using a jeweler's circular saw 0.2 mm thick. Water makes an excellent lubricant for this process. It is thus possible to get about three sections per millimeter. The polished surface is next attached to a "Plexiglass" slide, using the partial polymer as a cement. The whole system is placed in a 40° C. oven until polymerization of the cement is complete, about six hours. The thick section adhering to the slide is now ground to the desired thinness, using the same abrasives as before. Parallel grinding is made easier and excessive thinness avoided if the two ends of the slide are wrapped with paper-thin copper foil. This grinding of the second face takes slightly longer than the first and may consume ten or fifteen minutes, in the course of which the section is viewed from time to time under a low power microscope. With very little practice a thinness and evenness of the section equalling that produced with a microtome may be attained. Following this grinding a cover glass is affixed by the usual method.

Mass staining of tissues sectioned by this technique is usually quite satisfactory. However, it is surprising to note that staining after section is also possible. The stains used should be relatively strong, for example, safranin or methylene blue, and the times necessary are somewhat longer than usual. The results are nevertheless excellent. If this method is to be used it is advisable to stain the surface exposed by the first grinding, in order that the color of the section may be utilized to judge its thickness during the second grinding.

By this technique one can, with simple equipment, obtain excellent sections not only of ordinary tissues but also of those not easily treated by the standard microtome technique. The time required to get a single section is somewhat longer than that demanded by the paraffin method, due to the time occupied in polymerization; however, the time spent by the operator in manipulation is nearly the same. We hope to amplify this bald description with further details in a more complete publication elsewhere.

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THE STAINING OF ACID-FAST TUBERCLE BACTERIA

MORE bacteria in a given sample are revealed by the fluorescence technique[1] than by the Ziehl-Neelsen method. Our observations confirm the reports of the German investigators, especially Didion.[2] The increased number is a real difference beyond the increased visibility of the bacteria with the fluorescence technique. Of the many compounds isolated from tubercle bacilli by Professor Rudolph Anderson and his associates, only mycolic acid[3] was found to be acid-fast. Professor Anderson very kindly furnished me with some pure mycolic acid.

A small amount was melted on clean slides and stained in carbol-auramin and in carbol-fuchsin. The auramin stained the mycolic acid intensely yellow at room temperature, and the combination resisted destaining in acid alcohol. After three days and nights destaining, most of the auramin remained in the sample. On exposure to ultraviolet light an intense bright yellow fluorescence was observed.

The mycolic acid was stained with difficulty by the carbol-fuchsin of the Ziehl-Neelsen method and easily destained in acid-alcohol (0.5 per cent. HCl) to about one third of the original intensity. Slightly better staining occurred when the acid was stained with steaming carbol-fuchsin, but the mycolic acid is not strongly acid-fast to fuchsin.

Mycolic acid itself is weakly fluorescent, showing pale blue-white and may have caused the fluorescence observed by Kaiserling[4] and by Arloing *et al.*[5] in tubercle bacteria. Melting and cooling the acid markedly reduces the autofluorescence of the acid.

The observations suggest that the acid-fast staining of the tubercle bacilli may be due to mycolic acid. The firmer combination of mycolic acid with carbol-auramin than with carbol-fuchsin may explain why more bacteria are revealed by the fluorescence technique, using the former, than by the Ziehl-Neelsen method, using the latter.

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SPENCER LENS COMPANY

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